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The propositional basis of cue-controlled reward seeking

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Abstract

Two experiments examined the role of propositional and automatic (ideomotor) processes in cue elicited responding for rewarding outcomes (beer and chocolate). In a training phase, participants earned either chocolate or beer points by making one of two button-press responses. Rewards were indicated by the presentation of chocolate and beer pictures. On test, each trial began with a picture of beer or chocolate, or a blank screen, and choice of the beer versus chocolate response was assessed in the presence of these three pictures. Participants tended to choose the beer and chocolate response in the presence of the beer and chocolate pictures, respectively. In Experiment 1, instructions signalling that the pictures did not indicate which response would be rewarded significantly reduced the priming effect. In Experiment 2, instructions indicating that the pictures signified which response would *not* be rewarded resulted in a reversed priming effect. Finally, in both experiments, the priming effect correlated with self-reported beliefs that the cues signalled which response was more likely to be reinforced. These results suggest that cue elicited response selection is mediated by a propositional belief regarding the efficacy of the response-outcome relationship, rather than an automatic ideomotor mechanism.

Keywords: stimulus control; associative processes; motivation; addiction; rewards.

Cue-reactivity research has shown that drug cues are a key factor in the maintenance of drug addiction. Drug-related stimuli provoke craving (Sayette & Tiffany, 2013), drug-seeking (Hogarth & Chase, 2011) and drug-taking behaviour (Hogarth, Dickinson, & Duka, 2010), and contribute to maintenance and relapse to drug use in the natural environment (Shiffman, 2009). One method that has been used extensively in the laboratory to explore the mechanisms underlying cue-controlled behaviour is the Pavlovian-Instrumental Transfer (PIT) paradigm (see Holmes, Marchand, & Coutureau, 2010). Here, two instrumental responses (R1 and R2) are established as predictors of two distinct rewarding outcomes (O1 and O2) in a concurrent training phase (R1-O1, R2-O2). Separately, two Pavlovian cues (S1 and S2) are trained to predict the same outcomes (S1-O1, S2-O2). Finally, in the transfer test conducted in extinction, the cues are presented for the first time in conjunction with the instrumental responses. It is typically found that the presentation of the Pavlovian cues selectively facilitates the response paired with the same outcome (S1 and S2 promote R1 and R2 respectively). This outcome-selective PIT effect reflects an interaction between associative learning processes, where the Pavlovian stimulus (e.g. S1) enhances the instrumental response (e.g. R1) that shared a common outcome (O1).

The ability of cues to control responses in this way is a robust finding that has been demonstrated in both rodents (e.g., Colwill & Rescorla, 1988; Holland, 2004) and humans (e.g., Hogarth, Dickinson, Wright, Kouvaraki, & Duka, 2007) and is thought to be a key factor underlying the maintenance and relapse of chronic drug addiction (Belin, Jonkman, Dickinson, Robbins, & Everitt, 2009). Understanding the fundamental associative and motivational processes underlying cue-controlled behaviour is therefore crucial for the development of effective clinical treatments for addiction disorders.

Although substantial progress has been made in recent years to elucidate the neural substrate of phenomena such as PIT (e.g., Corbit & Balleine, 2005, 2011), explanations at the psychological level remain a matter of debate. The PIT effect specifically has been explained through both link-based and propositional approaches. Within the link models, there is some debate regarding the associative structure underlying PIT (Colwill &

Rescorla, 1990; Balleine & Ostlund, 2007; de Wit & Dickinson, 2009; Bradfield & Balleine, 2013; Cohen-Hatton, Haddon, George, & Honey, 2013; Hogarth et al., 2014), but the dominant approach is founded upon outcome-response (O-R) theory. Here, instrumental learning is suggested to result in the formation of a link between the mental representation of the response and outcome as a consequence of instrumental (R-O) training. This link is bidirectional, such that activation can also pass from O to R (via the O-R link). This approach is intimately related to ideomotor theories of motor action, in which an individual imagines (or retrieves) a representation of a particular outcome or goal, which triggers the execution of the response sequence required to obtain that goal in a relatively automatic or unconscious way. Such action-effect associations are thought to develop when learning about the outcome of a particular response (Hommel, Müsseler, Aschersleben, & Prinz, 2001; Eder & Hommel, 2013; Eder, Rothermund, De Houwer, & Hommel, 2014). Importantly, O-R (ideomotor) theories argue that not only the direct perception, but also the mere thought of the outcome will prime the associated motor response. This same idea can be seen in the PIT literature. Here, the presentation of the stimulus S during the transfer test is suggested to trigger a memory (or thought) of the associated outcome O, which in turn, automatically primes the associated instrumental response R. Hence, the outcome representation mediates the stimulus-response (S-R) relationship through an ideomotor S-O-R associative chain.

There is a large body of literature to support ideomotor theory (for a recent review see Hommel, 2013). Elsner and Hommel (2001), for example, trained participants to perform left and right button press responses (R1 and R2), which were each followed by either a high or low tone (O1 and O2 respectively). In a subsequent test phase, these tone outcomes were presented initially as imperative stimuli. Participants were required to make one response (as quickly as possible) when the high tone was presented and the other response when the low tone was presented. Two groups were compared. In the action-consistent group, the mapping of response to outcome was the same as in the training phase; presentation of O1 signalled that participants were to execute response R1, and O2 signalled R2. For the action-inconsistent group, the mapping was reversed (O1-R2 and O2-R1). Critically, participants in the action-inconsistent group were significantly slower to produce the correct response than the control, action-consistent group. This is generally regarded as evidence for the automatic nature of action selection. Thus, in the action-inconsistent group, the automatic activation of R1 by O1 (due to the O1-R1 binding) interfered with the execution of the instructed R2 in response to O1.

Elsner and Hommel's (2001) experiment shows that, at least when neutral stimuli are used, learning an R-O relationship can impact on the speed with which the response R is executed in the presence of the outcome O. This lends credence to the S-O-R model of PIT, in which the S activates the O via an S-O link, and this activation then triggers the R via the O-R (ideomotor) link. The current experiments were designed to test whether an ideomotor mechanism also plays a role in the context of choice of valued rewards such as those used in PIT tests (specifically, beer and chocolate).

Although there is no direct evidence for the ideomotor mechanism in PIT, many studies have provided evidence for automaticity more generally. In these studies, one of the rewarding outcomes is devalued prior to the transfer test. While instrumental responding typically declines for an outcome that has been devalued, the PIT effect has been shown to be insensitive to this manipulation. That is, the ability of a reward cue (S) to increase responding (R) for the paired outcome (O) is independent of the current value of that outcome (Rescorla, 1994; Holland, 2004; Corbit, Janak, & Balleine, 2007; Hogarth & Chase, 2011; Hogarth, 2012; Watson, Wiers, Hommel, & de Wit, 2014). This result is generally regarded as evidence to suggest that PIT is automatic, not goal-directed (de Wit & Dickinson, 2009), and is consistent with an S-O-R model. According to the S-O-R model, the stimulus in a PIT test activates a representation of the *identity* of the outcome (whatever its value), and this is sufficient to trigger the associated instrumental response (Rescorla, 1994). If the analysis above is correct, PIT might also be insensitive to other post-training manipulations that aim to tap into high-level, propositional processes, for example verbal instructions. The current experiments were designed to test this prediction.

The alternative to the automatic link-based mechanism described above is that PIT is the consequence of a goal-directed propositional process. On this view, responding is a *deliberate choice* driven by explicit knowledge that in the presence of the Pavlovian stimulus, the instrumental response is more likely to produce the rewarding outcome. This approach leaves open the question as to how the choice of outcome (O) is translated into observable behaviour (R), although it is widely assumed that humans act to achieve desired outcomes in a goal-directed manner. It may be that, once the choice of outcome has been made, production of the response is the consequence of the same ideomotor process postulated by the S-O-R theory (Hommel, 2013). What distinguishes this goal-directed process from the S-O-R mechanism is that simply having the outcome O in mind – activating thoughts about the outcome (perhaps via the presentation of a reminder stimulus S) – is not sufficient to trigger the response. Rather, the outcome in question must be chosen as the one to be obtained. It is only this choice that will allow behaviour to be generated. The focus of the current paper is whether simple activation of an outcome is sufficient to produce behaviour, as proposed by S-O-R theorists, or whether deliberate choice based on propositional knowledge is necessary.

There has been remarkably little discussion of the role of propositional processes in the PIT literature until recently. One of the main reasons for this is because, as discussed above, PIT is insensitive to outcome devaluation. There is now, however, evidence to support the idea that human PIT is not automatic. For example, PIT effects are only found in participants who report explicit knowledge of the predictive relationship between the stimulus and the outcome (e.g. Hogarth, Dickinson, Wright, Kouvaraki & Duka, 2007; Trick, Hogarth, & Duka, 2011; Lovibond, Satkunarajah, & Colagiuri, 2015). This dependence on explicit knowledge is entirely consistent with the propositional account. S-O-R theory, on the other hand, assumes that PIT is at least partly driven by an automatic, unconscious psychological process, so cannot readily account for why PIT effects would be restricted to participants who possess explicit knowledge of the relevant contingencies. To explain this, the S-O-R account must assume that ideomotor learning drives the PIT effect, and that propositional knowledge of the contingencies is merely an epiphenomenon that is correlated with ideomotor learning.

Evidence for the causal role of propositional processes in PIT comes from Hogarth et al.'s (2014, Experiment 3) instructional manipulation. In this task, two instrumental responses were established as predictors of either beer or chocolate points (R1-O1, R2-O2). Response choice was then tested in the presence of pictorial beer or chocolate stimuli. The participants in the control group showed a standard outcome-selective PIT effect, whereby the presentation of a reward cue (a picture of beer or chocolate) selectively increased choice of the response that had been paired with that reward in training. Importantly, an experimental group were instructed prior to the transfer test that the 'Pictures do not indicate which key is more likely to be rewarded'. The PIT effect was attenuated in participants given these instructions. Furthermore, within the instructed group, participants who believed the instructions showed absolutely no PIT effect. Those who retained some belief that cues signalled the effective R-O relations, however, continued to show a PIT effect. Hogarth et al.'s finding suggests that the non-instructed group's responses were mediated by the belief that the picture signalled which response would be rewarded. The experimental (instructed) group did not possess this belief, and so the pictures did not influence response choice.

If a verbal instruction can lead participants to abandon their belief that the cues signal which response will be reinforced, and this also abolishes the PIT effect, then it seems unlikely that PIT is the result of the automatic activation of an instrumental response via a chain of associative links (S-O-R theory). Hogarth et al.'s (2014) data, therefore, support a propositional account of PIT. However, it is possible that both propositional and ideomotor processes play a role, but that Hogarth's procedure was not optimised to establish ideomotor learning. The present experiments explored this approach further by probing instructional sensitivity under conditions that might more directly test for evidence of an O-R link mechanism. In the PIT studies from which evidence for propositional processes is drawn (Hogarth et al., 2014), the stimulus used in the PIT test had never been directly paired with the outcome that the response produced. Specifically, one response earned the outcome text 'One beer point' whereas the other response earned the outcome text 'One chocolate point'. By contrast, the beer stimulus that was presented on test was a picture of two glass jugs of beer being clashed together, and the chocolate picture was a close up of a chocolate block. Although these cues will have been paired with a range of beer and chocolate outcomes in the past, they should never have been paired with the specific textual outcomes earned by the response. Thus, for a PIT effect to occur, the beer stimulus (for example) must retrieve the general category of beer, which generalises to retrieve the beer outcome ‘One beer point’ and subsequently primes the response associated with this specific outcome. It is possible that the indirect relationship between the S and the R (mediated by a chain of two related Os) in the PIT test favours a propositional process over a more automatic O-R mechanism. By contrast, in Elsner and Hommel’s (2001) demonstration of the ideomotor mechanism, the S presented in the PIT test was the O with which the R had been paired during training. Therefore, a more direct test for the contribution of O-R links in choice of valued rewards would be to present the actual outcome O with which the response R was paired during instrumental training. Perhaps these conditions would be better suited to demonstrate the operation of an automatic ideomotor process.

In summary, there is reaction time evidence that neutral outcomes (tones) can prime responses with which they have been paired in an apparently automatic fashion (Elsner & Hommel, 2001). These results are consistent with the operation of an O-R or ideomotor mechanism. The O-R mechanism has also been suggested to account for choice of valued rewards in PIT (de Wit & Dickinson, 2009; Eder & Hommel, 2013). There is, however, little direct evidence for the role of an O-R link mechanism in choice of the valued rewards typically used in PIT tasks. Moreover, recent PIT experiments using such rewards suggest that cue control is mediated by a propositional process (Hogarth et al., 2014); participants appear to interpret the presence of the cue as indicative of which response-outcome relation will be effective. The aim of the current experiments was to further explore whether stimuli can exert automatic control over instrumental responding, using a task that might encourage greater expression of the O-R link mechanism.

Experiment 1

The first experiment was very close in design to the instructional PIT experiment of Hogarth et al. (2014, Experiment 3). Consistent with past literature (e.g., Hogarth et al., 2007; Hogarth & Chase, 2011, 2012; Hogarth, 2012; Hogarth, Field, & Rose, 2013), a concurrent choice procedure using symbolic rewards was employed during training to establish the R-O contingencies. Such procedures have previously produced reliable correlations between preferential drug choice and dependence using a range of reinforcers including tobacco (Hogarth & Chase, 2011; Hogarth, 2012) and cocaine (Moeller et al., 2009, 2013). This method was therefore chosen because it effectively models reward choice in subclinical populations whilst maintaining a high level of experimental control. Thus, in a single training phase, two instrumental responses (R1 and R2) were trained to predict beer and chocolate outcomes (O1 and O2). These outcomes were represented as a compound of the text "You earn" and a picture of the outcome, beer or chocolate. Crucially, the same picture outcomes were then used as stimuli on the PIT test trials. This notionally favours ideomotor control because these pictures previously served as outcomes produced by the instrumental responses during concurrent choice training. This contrasts with Hogarth et al.'s (2014) experiment, where instrumental responses were reinforced with textual reward points and no pictures during training. In the current study, we anticipated that O1 would increase choice of R1, and O2 would increase R2. As in Hogarth et al.'s study, half the participants were instructed prior to the test phase that the pictures did not indicate which response would be rewarded. Also consistent with Hogarth et al., these pictures were presented alone before instrumental responding was tested in their presence (responding was not time limited). If the pictures promote responding via a propositional process, then the instructions should eliminate the cueing effect of the pictures. If, however, the pictures prime responding via an automatic (O-R) mechanism, then the instructions will have no impact. Finally, a dual-system account (McLaren et al. 2014) in which both propositional and link-based processes contribute to behaviour, would predict a decrease in the cue-control effect, due to the removal of the propositional contribution. However, some degree of automatic cue control would persist even in those individuals who report belief in the instructions.

Method

Participants

Fifty seven participants (22 males, 35 females), aged between 18 and 48 (*M* = 21.04, *S.D.* = 4.94), completed the experiment in exchange for course credits. At the start of the experiment, participants were randomly allocated to the instruction or no instruction condition. Participants provided informed consent and the experiment was approved by the School of Psychology ethics committee at Plymouth University.

Apparatus

The task was programmed in E-Prime (Psychology Software Tools, Inc.; pstnet.com) and was presented in an individual laboratory cubicle on a 22-inch computer monitor. A 660ml bottle of Beck's beer and a 45 gram Cadbury's Dairy Milk chocolate bar served as the physical reinforcers that participants believed they could earn points towards during the task.

Procedure

After providing demographic information, participants were shown the bottle of beer and the chocolate bar and were told that they could try to win them during the experiment. On-screen instructions read, "In this task, you can earn the beer and chocolate in front of you by pressing the left or right arrow keys. You will only win rewards on some trials. Press any key to begin." These rewards were hidden when the task began.

Concurrent choice training. The computer task began with 24 trials of concurrent choice training, in which the response-outcome relationships were established. Each response was paired with either beer or chocolate, and this was counterbalanced between subjects. Each trial began with a symbolic representation of the choice, centrally presented on-screen as "← or →". After a left or right arrow key response was made, this was immediately replaced by the statement "You earn" and a picture of either beer (two glasses of beer being clashed together) or chocolate (a close up of a chocolate block). Each outcome was scheduled to be available on 50% of the trials, which were randomly distributed throughout training. When subjects selected a response for an outcome that was not available, they were presented with the text, "You win nothing" and a grey (blank) rectangle stimulus. This stimulus subsequently served as the blank stimulus in the transfer test. All outcomes were presented for 1500ms and were followed by an intertrial interval (ITI) that varied randomly between 750 and 1250ms.

Contingency knowledge test. A short contingency knowledge test followed the concurrent choice training phase. Participants were asked two questions, "Which key earned beer (or chocolate), the left or the right arrow key? Please choose carefully". These questions were randomly ordered and were separated by a 750-1250ms ITI.

Test phase. Participants then completed the test phase, after reading the following on-screen instructions: "In this part of the task, you can earn beer and chocolate by pressing the left or right arrow key in the same way as before. You will only be told how many of each reward you have earned at the end of the experiment. Also, sometimes a picture of beer or chocolate will be presented before you choose the left or right arrow key. Press any key to begin." Participants in the instruction condition were also told, "Pictures do not indicate which arrow key is more likely to be rewarded!" This appeared both on the initial instruction page (beneath the main instructions) and also at the bottom of the screen continuously throughout the test period. Each trial started with the presentation of the beer, chocolate or blank outcome pictures that were used during concurrent choice training. These were presented for 3s before an instrumental response (also represented on-screen as "← or →"), was required. Responding, tested in the presence of the pictures, was not time limited. The three picture cues were scheduled to be presented 16 times each throughout the test phase (48 trials in total). There were eight cycles of six trials. Every six-trial cycle contained two trials of each cue presented in a random order. The trials were separated by an ITI of 750-1250ms. The dependent variable was the percent choice of the beer key over the chocolate key (where 50% = indifference) in the presence of each stimulus.

Expectancy questions. Finally, participants answered two expectancy questions. These were prefaced with the instructions, “We would now like to examine your thoughts about the beer and chocolate pictures. Please think carefully about your answers. Press any key to begin.” The beer and chocolate pictures were then presented in a random order with the on-screen question, “When this picture was presented, to what extent did you think that the beer (or chocolate) key was more likely to be rewarded?” Participants were required to press a key from 1-7, where 1 and 7 represented “Not at all” and “Very much”, respectively. The statements were randomly ordered and were separated by an ITI varying between 350-750ms. Finally, participants were thanked, offered a small chocolate reward and were fully debriefed.

Results

Two participants from the non-instructed group were excluded for failing to correctly report the response-outcome relationships during the contingency knowledge test, leaving 28 participants in the instructed group, and 27 participants in the non-instructed group.

Test phase

Figure 1 shows the mean percent choice of the beer key versus chocolate key following each of the three pictures (beer, blank and chocolate) within both groups. The cueing effect is indicated by the extent to which beer responses are increased by the presence of the beer picture, and decreased by the presence of the chocolate picture, relative to the “no stimulus” blank condition. The graph indicates that a cueing effect was present in both groups, but was reduced in the instruction group. A mixed ANOVA confirmed this interpretation. There was a main effect of stimulus, F (1, 53) = 78.37, $p < .001$, but not of instruction, $F < 1$. There was also an interaction between stimulus and instruction, F (1, 53) = 13.78, $p < .001$. Furthermore, there was a simple effect of stimulus in both the non-instruction, $F\ (1, 53) = 77.53$, $p < .001$, and instruction group, $F\ (1, 53) = 13.46$, $p < .001$.

(Figure 1 about here)

Expectancy ratings

Figure 2 shows the effect of instruction group on self-reported expectancy of the cued outcome, that is, the extent to which participants expected a beer reward following a beer picture and a chocolate reward following a chocolate picture. The graph suggests that expectancy ratings were not reduced by the instruction. A mixed ANOVA confirmed this, revealing no main effect of instruction group, $F < 1$. There was a main effect of outcome, $F\ (1, 53) = 7.61$, $p < .01$, however, and a significant interaction between outcome and instruction group, $F\ (1, 53) = 7.61$, $p < .01$. There was a simple effect of outcome in the instruction group, $F\ (1, 53) = 15.49$, $p < .001$, but not in the non-instruction group, $F < 1$. There was no main effect of instruction group with respect to expectancy reports for either the beer stimulus, $F\ (1, 53) = 1.44$, $p > .05$, or the chocolate stimulus, $F < 1$, suggesting that neither cueing effect was sensitive to the instruction.

(Figure 2 about here)

One sample *t*-tests were conducted comparing the average expectancy score of each instruction group to the mid-point value of four. This median value indicates an expectation that both outcomes are equally likely to be rewarded, regardless of the cue. This comparison revealed a significant difference in both the non-instruction, $t\ (26) = 2.89$, $p < .01$, and the instruction condition, $t\ (27) = 3.48$, $p < .01$, suggesting that both groups expected the cued outcome to be rewarded. To further explore the relationship between expectancy ratings and the cueing effect, each instruction group was divided into ‘high’ and ‘low’ expectancy subgroups. Subjects scoring above four on the expectancy measure (averaged across outcomes) were assigned to the high expectancy group (*N* = 41), while all others were classified as low expectancy subjects (*N* = 14). Figure 3 shows the cueing effect in each instruction and expectancy sub-group. The graph suggests that a cueing effect was only observed in participants who reported high expectancy ratings, and this was reduced in the instructed condition. A three-way ANOVA on the instruction group, expectancy group and stimulus variables revealed a main effect of stimulus, F (1, 102) = 38.21, $p < .001$, but not of expectancy group, F (1, 51) = 1.47, $p > .05$, or instruction group, $F < 1$. There was an interaction between stimulus and instruction group, F (2, 102) = 10.18, $p < .001$, and between stimulus and expectancy group, F (2, 102) = 29.16, $p < .001$, but not between the instruction and expectancy groups, $F < 1$. Finally, there was a three-way interaction between stimulus, instruction and expectancy group, F (2, 102) = 4.34, $p < .02$.

The significant three-way interaction between stimulus, instruction and expectancy group was further explored with an analysis of simple main effects. Collapsed across instruction groups, there was a large effect of stimulus in the high expectancy group, F (2, 78) = 116.44, $p < .001$, but not in the low expectancy group, $F < 1$, indicating that the cueing effect was only present among participants who reported high expectancy ratings. Neither expectancy group showed an effect of instruction, Fs < 1. There was a significant interaction between stimulus and instruction in the high-expectancy group, F (2, 78) = 24.32, $p < .001$, suggesting that the cueing effect was reduced by the instruction even for participants who reported a high expectancy of the S:R-O contingencies. By contrast, there was no interaction between stimulus and instruction among the low-expectancy group, $F < 1$. Furthermore, both the instructed, F (2, 36) = 16.65, $p < .001$, and non-instructed, F (2, 36) = 146.52, $p < .001$, groups showed simple effects of stimulus in the high-expectancy group, indicating that a cueing effect was present in both instruction groups. This was not observed in participants reporting low expectancies, regardless of the instruction condition, Fs < 1.11, ps > .36.

The failure to observe a cueing effect in the low expectancy group prompted a Bayesian analysis on these data. A beer cueing effect was calculated for each participant by subtracting the percent choice of the beer key on non-cued trials from the percent beer choice on beer-cued trials. Conversely, a chocolate cueing effect was calculated by subtracting the percent beer choice on chocolate-cued trials from the percent beer choice on non-cued trials. The overall cueing effect represents the mean of the beer and chocolate cueing effects. The mean cueing effect (mean=2.23, SEM=2.03) did not significantly differ from zero in the low expectancy group, t (13) = 1.10, $p > .05$. The alternative hypothesis here predicts a cueing effect in participants who reported low expectancy ratings. That is, the mean cueing effect should be greater than zero. Bayes factors below 0.3 and above 3 are evidence for the null and the alternative hypothesis, respectively, while values in between indicate that the data are insensitive (Dienes, 2011). Using a uniform distribution ranging from zero to 50 (the maximum plausible cueing effect), we calculated a Bayes factor of 0.16. This is support for the null hypothesis, suggesting that the cueing effect was abolished in participants reporting low expectancy ratings.

(Figure 3 about here)

Figure 4 shows a positive correlation between expectancy ratings and the cueing effect (both averaged across beer and chocolate outcomes). Overall reward expectancy strongly correlated with the overall cueing effect, $r = .60$, $p < .001$, and this correlation was significant in both the instruction, $r = .52$, $p < .01$ and the no instruction, $r = .71$, $p < .001$, condition.

(Figure 4 about here)

Discussion

In the standard non-instructed group, pictures of beer and chocolate promoted responses that had been followed by those pictures during training – a typical outcome cueing effect. The instructed group were told that the pictures did not indicate which response would be rewarded in the test phase. The cueing effect was significantly reduced, but not eliminated, as a consequence of these instructions. The result is a replication of Hogarth et al.'s (2014) Experiment 3, in which a similar instruction also partially eliminated the PIT effect across the sample as a whole. Collectively, this sensitivity to instructional manipulations indicates that the priming effect seen in both PIT and O-R paradigms is at least partially governed by a propositional process.

Although the cueing effect was reduced in the instructed group, it was not eliminated entirely. This attenuation of the cueing effect is predicted by dual-process models, where the propositional component is sensitive to the instruction but the automatic component is not. However, it is also clear that expectancy ratings were not reduced in the instructed condition; both groups reported a belief that the stimulus signalled which response was more likely to be rewarded. While the propositional approach predicts a tight coupling between expectancy ratings and cueing effects, the dual-process model does not. Indeed, the latter approach would predict a residual cueing effect even amongst participants reporting low expectancies, because the automatic component that drives response selection is independent of conscious expectancies. Further analysis of the expectancy ratings, however, suggested that the cueing effect was completely abolished in participants who reported low expectancy ratings (regardless of the instruction). This was confirmed by the Bayes analysis, which provided support for the null hypothesis. This is consistent with the result reported by Hogarth et al. (2014), where PIT was abolished only in participants who reported beliefs that were consistent with the instructional manipulation. Furthermore, the current study revealed a positive correlation between the strength of expectancy ratings and the size of the cueing effect in both instruction groups. Together, these data suggest that the cueing effect is mediated by conscious, verbalizable beliefs about the signalling role of the stimulus. The instruction used in the present study only weakly influenced these beliefs, and so the cueing effect was only partially sensitive to the instruction. Experiment 2 was therefore designed to test whether a stronger instructional manipulation would modify expectancies more successfully, and subsequently produce greater sensitivity to the instruction.

Experiment 2

Experiment 2 used a reversal instruction to provide a stronger test of the propositional basis of outcome-primed responding. Participants were instructed that the pictures presented on test signalled which response would *not* be reinforced. If cue-elicited responding is governed by propositional knowledge, these instructions will reverse the pattern of responding.

The residual cueing effect seen in the instructed group of Experiment 1 was explained in two ways, which each make distinct predictions with respect to the reversal instruction used in the current experiment. Firstly, it is possible that the residual cueing effect seen in Experiment 1 was the result of a propositional process. On this view, the instructions were only weakly effective in reducing participants' beliefs about the role of the reward cues in signalling which R-O contingency would be reinforced, and so the instructions only partially reduced the PIT effect. The reversal instructions used in the current design give very specific propositional directions as to the cues' relations to the response-outcome contingencies, which are the opposite of any ideomotor O-R links that might have been established in training. The propositional approach predicts that the cueing effect will be completely reversed in the instructed group.

By contrast, the dual-system account suggests that an automatic O-R mechanism may have been responsible for the residual cueing effect seen in the instructed group. This predicts that the reversal instructions will produce a partial reversal in responding. The propositional component of the response will reverse, but the automatic component will produce responding in line with the trained contingencies. These two components will combine in the instructed group to produce a net effect somewhere between the standard cueing effect and complete reversal.

One last issue that was investigated in Experiment 2 was the temporal delay between picture presentation and opportunity to respond on test. A three second delay was imposed between the stimulus onset and the response prompt in Experiment 1. Of course, such delays are common in the real-world environment. The interval between a smoker noticing a lighter in the office and walking to the newsagent to buy cigarettes, for example, provides ample opportunity for a propositional process to intervene and halt an automatic O-R process. It is possible, however, that cued responding would be less sensitive to instructional manipulations without this forced delay, due to a relative preponderance of the O-R process. Experiment 2 aimed to test this by randomly allocating participants to a slow or fast group, which differed in the length of the delay between the stimulus onset and the required response. As in Experiment 1, the slow group was required to wait for at least 3s after the stimulus onset before making a forced-choice instrumental response. In the fast condition, this duration was reduced to 300ms. Studies in the affective priming literature (e.g., Hermans, De Houwer, & Eelen, 2001) have shown that a stimulus onset asynchrony (SOA) of 300ms between the presentation of a prime and the onset of a target is sufficient to observe automatic priming effects (in the sense that they may occur independently of awareness of the priming stimulus). Controlled, propositional processes necessarily require working memory and time (Mitchell, De Houwer, & Lovibond, 2009). Automatic link-based processes, in contrast, can be expected to control behaviour even when time is limited. Consequently, evidence for an O-R link may be more likely to be observed under time constraints. In this case, reversal instructions may have less impact in the fast group, thereby revealing the operation of an O-R link.

Method

The method was the same as Experiment 1, except in the following respects.

Participants

Forty undergraduate psychology students (17 male, 23 female), aged between 18 and 40 (M = 21.69, *S.D.* = 4.81), completed the experiment in exchange for course credits. There were ten participants per group, with participants randomly allocated to the instruction and speed conditions.

Test phase

In place of the original instructions, the reversal groups were instructed that the "Pictures indicate which arrow key will NOT be rewarded!" This reversal instruction was presented with the main instructions before the test phase, and also at the bottom of the screen throughout testing. In the slow group, pictures presented on test were initially presented on their own for 3s before a response could be made in their presence (as in Experiment 1). For the fast group, the pictures were presented initially for 300ms before response choice was tested.

Results

All participants correctly reported the response-outcome mappings during the contingency knowledge test, suggesting that they had learned the R-O relationships. On this basis, no exclusions were made.

Test phase

Figure 5 shows the percent choice of the beer key according to the stimulus (beer, blank, chocolate), instruction (non-reversal, reversal) and speed (slow, fast) variables. As in Experiment 1, the cueing effect is indicated by the extent to which the beer stimulus increased, and the chocolate stimulus decreased, the percentage of beer over chocolate responses (relative to the blank stimulus). The graph suggests that a standard cueing effect was seen in the non-reversal groups, that the reversal instructions fully reversed this pattern and that the speed manipulation did not affect responding. A mixed ANOVA confirmed these impressions. There was no main effect of instruction or speed, *F*s < 1. There was also no main effect of stimulus, $F < 1$, suggesting that, across all groups, participants did not demonstrate any underlying bias towards the cued stimulus (that is, neither the reversal nor non-reversal effect was large enough to generate a main effect of stimulus across the sample as a whole). Most importantly, there was an interaction between stimulus and instruction, $F(1, 36) = 50.20$, $p < .001$, suggesting that the cueing effect was sensitive to the reversal instruction. There was no interaction between stimulus and speed, $F(1, 36) = 1.05$, $p > .05$, or instruction and speed, $F(1, 36) = 3.10$, $p = .09$. In neither instruction group was there an effect of speed (*F*s < 1.55, *p*s > .22), or an interaction between stimulus and speed (*F*s < 2.09, *p*s > .16). Lastly, there was no three-way interaction between stimulus, instruction and speed, $F(1.34, 48.31) = 1.07$, $p > .05$.

To determine whether the size of the cueing effects were equivalent between the instruction groups, we compared the cueing effects in each condition. For the non-reversal group, the cueing effect was calculated in the same way as in Experiment 1 (i.e. the cueing effect represents the increases in percent choice of the cued response above the blank condition). These calculations were reversed for the reversal group such that the overall cueing effect reflects the extent to which cues increased responding for the outcome *not* signalled by those cues, above the blank condition (i.e. the extent to which responding was consonant with the instructions). The size of these cueing effects in the reversal (mean=25.94, SEM=6.31) and non-reversal group (mean=33.60, SEM=5.57) did not differ, $t(38) = -0.91$, $p > .05$. Furthermore, we used a Bayesian analysis to aid interpretation of this null result. The alternative hypothesis predicts a standard PIT effect in the reversal instruction group, which would be indicated by a significant difference between the sizes of the cueing effects in each condition. According to the alternative hypothesis, the maximum standard cueing effect for the non-reversal group is 50, which would represent a perfect cueing effect. The highest plausible reversed cueing effect for the reversal group is -50, which would also indicate a perfect standard cueing effect (i.e. no sensitivity to the instruction). The maximum plausible effect size, therefore, would be 100. We used a uniform distribution ranging from zero to 100. With a sample mean difference of 7.66 and a standard error of 8.41, this produced a Bayes factor of 0.26. This is below the key value of one third, and so provides support for the null hypothesis.

In the overall ANOVA above, we reported that there was no effect of speed in either instruction group. This has particular importance for the reversal condition, because it was suggested that an automatic process (indicated by a standard cueing effect) would be more detectible without a forced delay. Consequently, we followed this null result with a Bayesian analysis, comparing the reversed cueing effect in each reversal condition. The size of the reversed cueing effect did not significantly differ in the slow (mean=25.94, SEM=9.34) and fast (mean=25.94, SEM=8.32) condition, $t < 1$. The alternative hypothesis predicts that the reversed cueing effect will be smaller in the fast condition, with maximum reversed cueing effect scores of 50 and -50 for the slow and fast condition, respectively. The maximal plausible effect size is, therefore, 100. We modelled the alternative hypothesis using a uniform distribution that ranged from zero to 100.With a sample mean difference of 0.002 and a standard error of 12.96, this produced a Bayes factor of 0.16, providing evidence for the null hypothesis.

(Figure 5 about here)

Expectancy questions

As in Experiment 1, a high expectancy score indicated high self-reported expectancy of the cued outcome. Expectancy ratings, shown in Figure 6, were analysed by mixed ANOVA according to the instruction (non-reversal, reversal), speed (slow, fast) and the outcome (beer, chocolate) variables. The reversal instruction reduced expectancies, $F(1, 36) = 17.63$, $p < .001$. No other main effects or interactions were found, *F*s < 2.23, *p*s > .14.

(Figure 6 about here)

Figure 7 shows the correlation between expectancy ratings and the overall cueing effect (calculated in the same way as in Experiment 1, i.e. positive scores reflect the extent to which cues augmented the response paired in training), averaged across the beer and chocolate outcomes. Self-reported average expectancy ratings positively correlated with the size of the overall cueing effect in the sample as a whole, $r = .63$, $p < .01$. Although this correlation was significant in the reversal group, $r = .42$, $p = .03$, and marginal in the non-reversal group, $r = .32$, $p = .09$ in isolation, an ANCOVA indicated that there was no significant difference between the two groups in the strength of the relationship between expectancy and the transfer effect, $F<1$.

(Figure 7 about here)

Discussion

Experiment 2 demonstrated that responding in the test phase was highly sensitive to the reversal instruction. The non-instructed group showed a standard cueing effect, replicating the results of the non-instructed group from Experiment 1. The reversal group showed the opposite pattern of responding, demonstrating a preference for responses that were not associated with the outcome pictures, but which were more likely to be reinforced according to the instructions. Most importantly, the absence of any main effect of stimulus across all participants shows that the reversal seen in the instructed group was complete. Furthermore, the reversal instruction was effective in reducing expectancy ratings for the cued outcomes, and these ratings positively correlated with the size of the overall cueing effect. This suggests that cue-elicited responding is governed by propositional beliefs about the role of the stimulus in signalling the status of response-outcome relationships.

Experiment 2 also tested whether shortening the duration between the stimulus onset and the subsequent instrumental response would reduce sensitivity to the reversal instruction, suggesting a greater preponderance of O-R automaticity in this condition. We found no evidence of this. Participants in the instructed groups showed an effect of stimulus in the opposite direction to the non-instructed groups, both in the slow and fast condition. It might be argued that this null effect was a result of a lack of power to detect an effect of delay. However, the comparison of the slow and fast reversal cueing effects was far from significant in the present data set, and it seems unlikely that greater power would radically change the pattern of data. Furthermore, the Bayesian analysis provided substantial support for the null hypothesis. Finally, the speed manipulation did not have any significant impact on the size of the cueing effect seen in the non-reversal group. Together, these results suggest that propositional processes facilitate cue-elicited responding.

General Discussion

The current experiments tested the propositional nature of cue-elicited instrumental responding through the use of instructional manipulations. Experiment 1 showed a reduction in outcome-primed responding following instructions that the pictures did not indicate which response would be rewarded. This replicates previous reports (Hogarth et al., 2014, Experiment 3) and suggests that instrumental responding is governed by propositional beliefs about the discriminative or hierarchical function of the stimulus in signalling the efficacy of the response-outcome relation. Furthermore, the cueing effect was entirely abolished in participants who reported no expectancy that the cues signalled the effective R-O relation. This also replicates the sub-group analysis of Hogarth et al. (2014). The only divergence was the failure of instructions to significantly reduce participants' self-reported expectancies that cues signalled the effective R-O relation. This discrepancy may be due to insensitivity of the continuous self-report measure in the current experiment compared to binary measure obtained in Hogarth et al. (2014).

Experiment 2 used a reversal instruction that was more effective in altering participants' beliefs about the signalling function of cues. Both the PIT effect and the expectancy ratings were highly sensitive to this instruction, suggesting that they are mediated by controlled processes. This instructional sensitivity was demonstrated regardless of whether a long or short delay was imposed prior to the response choice, despite there being a priori reason to anticipate preponderance of automaticity in the short condition (e.g., Hermans et al., 2001). Furthermore, in both Experiments 1 and 2, the size of the cueing effect correlated with self-reported expectations that the cue signalled which response would be rewarded. Together, these results suggest that, at least in O-R paradigms, performance is dominated by controlled reasoning processes[1].

This strong conclusion may be questioned by proponents of the dual-system view on two grounds. Firstly, some might argue that the instructional effects seen in the current study are also consistent with a link-based mechanism. That is, a model might be proposed in which verbal instructions can impact on the expression of associative links stored in memory. In Experiment 2, therefore, the instructions may have served to reverse the manner in which the associative links translated (automatically) into choice behaviour. It is not clear, however, that this is truly a dual-process model. If the automatic component can serve as an input into controlled processes, prior to the response being made, then that mechanism is not able to produce responses in an automatic fashion. Alternatively, some have proposed a more radical view in which language itself is link-based (in the sense used by associative learning theorists). It could be argued then, that because both the instructions and the postulated R-O links are represented in the same way, the fact that they interact with each

[1] It is possible that cueing effects (e.g., PIT) seen in non-human animals are the consequence of quite different psychological processes from those revealed in the current experiments. The current data do not speak to the issue of species generality.

other is not surprising. This also would not be a dual-system model, but a single-system link-based model of cognition. We would argue that the current findings (and instructional effects in general) are strongly indicative of a single system that controls performance in a non-automatic fashion.

There is also a second reason to question our conclusion that the cueing effect is propositional in nature. It is that our failure to find evidence for automatic priming may be peculiar to the procedure we used; other procedures may be more successful in this respect. For example, one of the most notable differences between Elsner & Hommel's (2001) experiments and the current Experiment 2 is that we did not require participants to respond as quickly as possible. It is possible that automatic ideomotor effects can be seen in decision making for valued rewards, but only under very specific circumstances, such as when there is little time to respond. Although extremely fast decision-making is not normally required in the natural environment, such a procedure may usefully tap into the impulsive nature of decision-making that is often reported in substance abusers (e.g., Mitchell, Fields, D'Esposito, & Boettiger, 2005). Similarly, a secondary task (which could mimic the distractions normally encountered in real-world situations) might reveal the operation of a more automatic, ideomotor process (Hogarth et al. 2012, 2013).

Another manipulation that might reveal a more automatic cueing effect is to increase the amount of instrumental training. Overtraining of the instrumental response is thought to promote habitual control in humans and therefore reduced sensitivity to outcome devaluation (Tricomi, Balleine, & O'Doherty, 2009). Participants in the present experiments received relatively modest training of the R-O contingencies and it is possible that they retained strong goal-directed control as a consequence. One argument against this possibility, however, is that past experiments using very similar parameters to those used here have demonstrated that outcome-selective PIT is insensitive to outcome devaluation. It is for this reason that the PIT effect is regarded as habitual or automatic (Hogarth & Chase, 2011; Hogarth, 2012; Hogarth, Balleine, Corbit, & Killcross, 2013). Probably the most interesting outcome of the current study, therefore, is that we observed instructional sensitivity (implicating goal-directed, propositional processes) using a procedure very similar to those used to demonstrate insensitivity of PIT to outcome devaluation (implicating automatic processes). This is a paradoxical result worthy of further analysis.

There is one straightforward way to reconcile the current instructional effects with previous observations that PIT is insensitive to devaluation. It is to postulate that insensitivity to outcome devaluation is, like the instructional effects, operating through a propositional process. It has been argued that action selection is determined by a function of outcome probability and outcome value. In particular, PIT stimuli have been suggested to modulate the expected probability that the response will produce the outcome, whereas internal incentive states (e.g. hunger, cravings, affective states) modulate the expected value of the outcome (Ostlund & Balleine, 2008; Hogarth & Chase, 2011; Hogarth, 2012; Hogarth & Troisi, 2015; Hogarth et al, 2015). These probability and value estimates concerning the outcome exert independent but summative effects on the tendency to perform the response. Thus, PIT cues (signalling outcome probability) may raise the performance of a particular response by a constant, even if the value of the expected outcome is low, and hence the PIT effect is relatively insensitive to devaluation. In other words, the PIT stimulus may increase the perceived efficacy of the associated response to such an extent that it outweighs the impact of outcome devaluation. This account can reconcile the apparently propositional nature of the PIT effect demonstrated here, with the apparent evidence for an 'automatic' PIT effect implied by its insensitivity to devaluation.

One last question is, to what extent do the current data relate to the clinical setting (see Hogarth, Maynard & Munafò, 2015)? One initial point to make is that the participants tested here were not selected on the basis of alcohol consumption, and we do not know whether the same pattern of data would be observed in participants who have been specifically screened for alcohol dependency. It should be noted, however, that outcome-selective PIT effects have generally been shown to be independent of the severity of drug dependence (Hogarth & Chase, 2011, 2012; Martinovic et al., 2014; Garbusow et al., 2014). It seems likely, therefore, that stimulus cueing effects may be observable in subclinical and dependent individuals alike. Probably the most important implication of the current data for the clinic relates back to the absence of devaluation effects seen in previous laboratory studies of PIT. These findings have usually been taken to imply that cueing effects (and therefore the cue-reactivity seen in clinical settings) are the consequence of an automatic, perhaps link-based mechanism. The suggestion has been, therefore, that interventions should target automatic associative link mechanisms. In contrast, the current data lend support to the idea that past devaluation experiments can be explained in terms of cues enhancing propositional expectancies concerning R-O probability, in which a temporary increase in outcome probability, signalled by the cue, trumps outcome value. This propositional process may also be at work in addiction disorders. As a consequence, strategies to reduce the impact of supposed associative links may not be as effective as is often thought.

This leads us to consider the difficulties of targeting maladaptive cue-elicited reward seeking. This is clearly a major challenge, irrespective of whether behaviour is mediated by an automatic or a propositional process. From a propositional perspective, eradicating the beliefs themselves would be perhaps the optimal solution. If individuals do not expect particular behaviours to produce rewards in the presence of certain cues, they will be less likely to engage in those behaviours. In the laboratory, this has been demonstrated using discriminative or hierarchical extinction procedures (Gámez & Rosas, 2005; Hogarth et al, 2014). However, this approach is limited by the inherent difficulty of extinguishing reward cues outside of the clinic. For example, the discriminative function of a bottle of beer may be successfully extinguished in a clinic. The alcoholic may no longer believe that the bottle predicts beer in that setting, and they may show little cue-reactivity. However, this belief is unlikely to translate to the real-world environment. Simply put, bottles of beer *are* good predictors of beer in the alcoholic’s usual environment, irrespective of whether they have been extinguished in an artificial setting. Thus, extinction procedures that lack realism are unlikely to alter propositional beliefs, and hence are unlikely to successfully modify behaviour. This highlights at least two fundamental difficulties of targeting cue-induced reward-seeking. First, although extinction procedures may effectively degrade beliefs about reward cues in a particular context, they may not translate to other environments. Second, they may not generalise to other, related stimuli, which may also trigger relapse. The implication here is that cues may continue to facilitate reward-seeking in the natural environment long after an individual has undergone an extinction procedure in a clinical setting.

To conclude, the present experiments found that cue-elicited instrumental responding was highly sensitive to instructional manipulations. Instructions that extinguished the signalling function of the pictures presented on test were effective in degrading the cueing effect. Similarly, participants demonstrated strong reversal of responding when instructed that the cue signalled the response that would not be rewarded. In our view, these data provide compelling evidence to suggest that, in the context of reward choices, outcome priming effects are governed by verbalizable beliefs about the signalling role of the stimulus.

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Figure Captions

Figure 1. The percentage of beer responses during the test phase according to instruction and stimulus variables. Error bars represent the standard error of the mean. 50% = indifference; > 50% = beer preference; < 50% = chocolate preference.

Figure 2. Self-reported expectancy of the cued outcome in each instruction group. Participants rated expectancy on a scale from 1-7, where 1 indicated that they expected the outcome "Not at all" and 7 "Very much". Error bars represent the standard error of the mean.

Figure 3. The percent choice of the beer key in each instruction and expectancy sub-group. Participants reporting a high expectation (scores greater than four) were allocated to the high-expectancy group, while all others were assigned to the low-expectancy group. Error bars represent the standard error of the mean.

Figure 4. The correlation between the overall cueing effect and expectancy ratings in each instruction group, averaged across outcomes (beer and chocolate).

Figure 5. The percent choice of the beer key in the presence of each stimulus, in each instruction and speed condition. Error bars represent the standard error of the mean. 50% = indifference; > 50% = beer preference; < 50% = chocolate preference.

Figure 6. Self-reported expectancy of the cued outcomes in each instruction group. An expectancy rating of 7 indicated that participants expected the cued outcome "Very Much", whereas 1 represented "Not at all". Error bars represent the standard error of the mean.

Figure 7. The positive correlation between expectancy ratings for the cued outcome and the overall cueing effect in each instruction group.

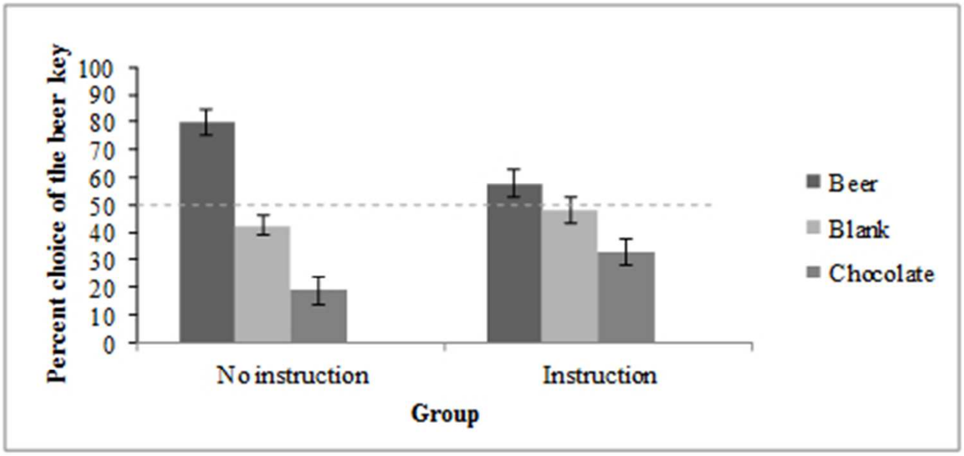

Figure 1. The percentage of beer responses during the test phase according to instruction and stimulus variables. Error bars represent the standard error of the mean. 50% = indifference; > 50% = beer preference; < 50% = chocolate preference.
129x61mm (96 x 96 DPI)

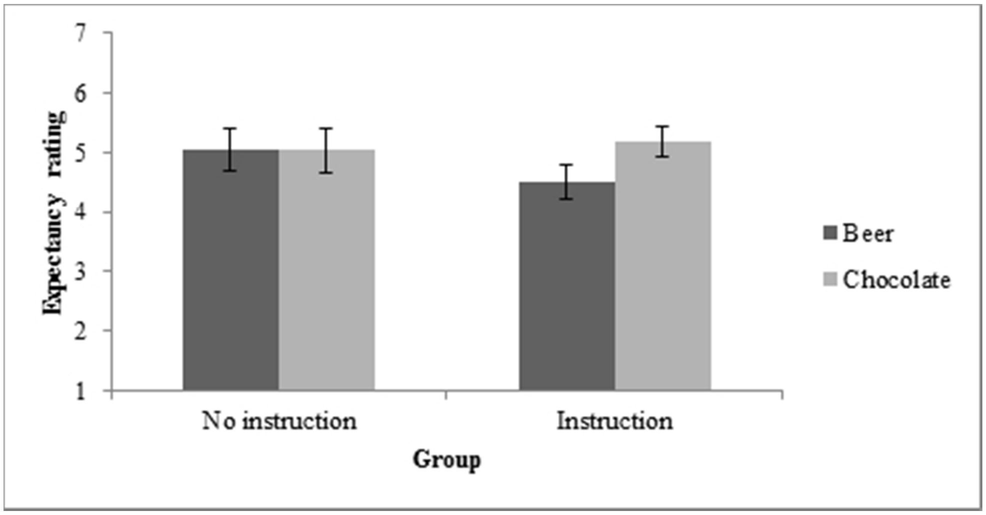

Figure 2. Self-reported expectancy of the cued outcome in each instruction group. Participants rated expectancy on a scale from 1-7, where 1 indicated that they expected the outcome “Not at all” and 7 “Very much”. Error bars represent the standard error of the mean.
129x66mm (96 x 96 DPI)

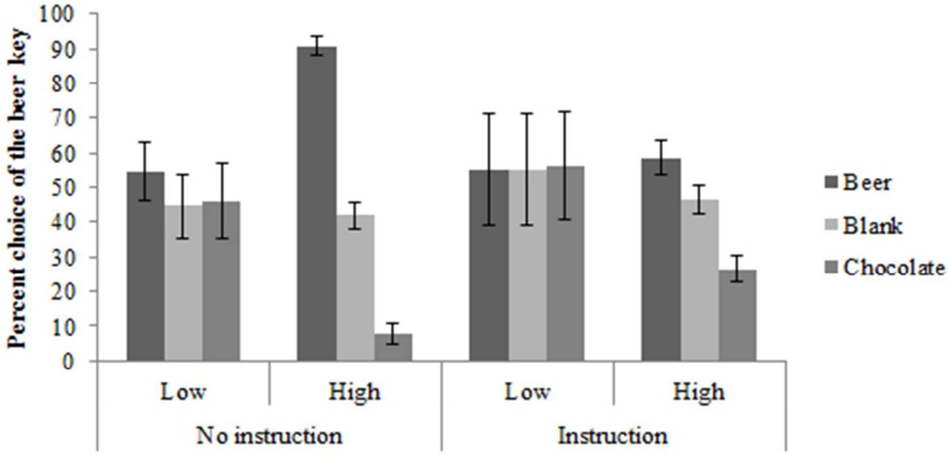

Figure 3. The percent choice of the beer key in each instruction and expectancy sub-group. Participants reporting a high expectation (scores greater than four) were allocated to the high-expectancy group, while all others were assigned to the low-expectancy group. Error bars represent the standard error of the mean.
139x66mm (96 x 96 DPI)

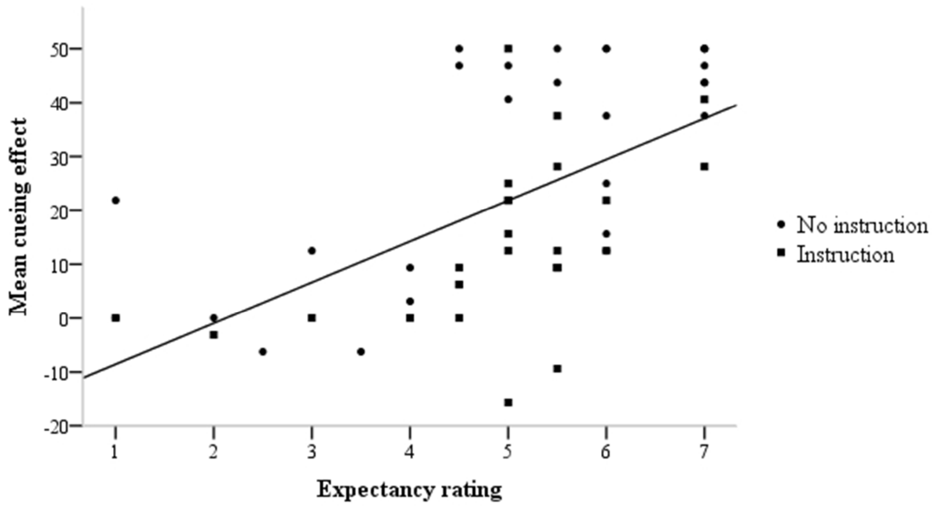

Figure 4. The correlation between the overall cueing effect and expectancy ratings in each instruction group, averaged across outcomes (beer and chocolate).
171x88mm (96 x 96 DPI)

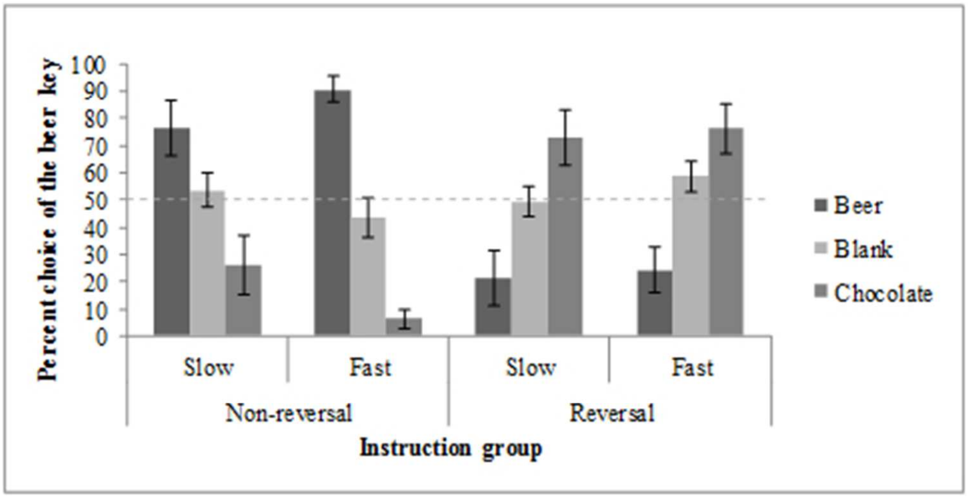

Figure 5. The percent choice of the beer key in the presence of each stimulus, in each instruction and speed condition. Error bars represent the standard error of the mean. 50% = indifference; > 50% = beer preference; < 50% = chocolate preference.
129x67mm (96 x 96 DPI)

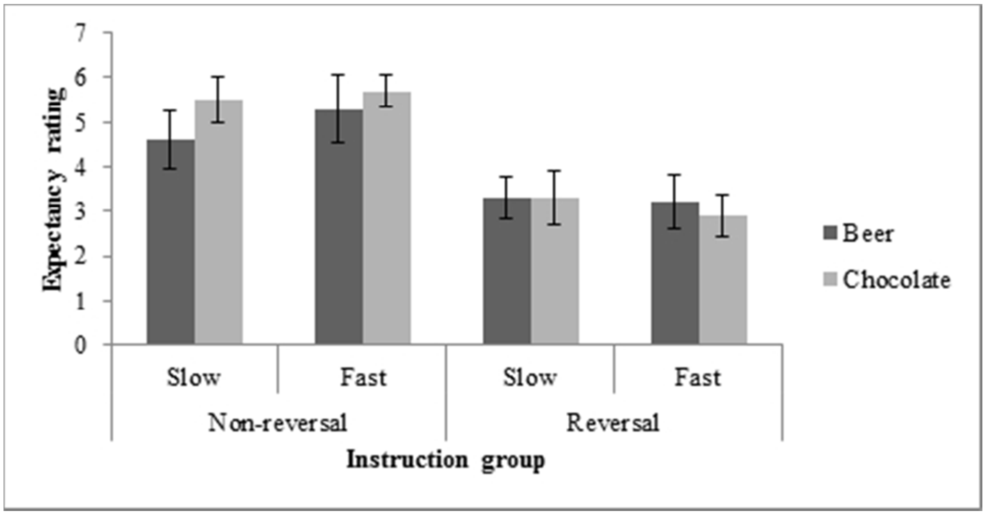

Figure 6. Self-reported expectancy of the cued outcomes in each instruction group. An expectancy rating of 7 indicated that participants expected the cued outcome "Very Much", whereas 1 represented "Not at all". Error bars represent the standard error of the mean.
129x66mm (96 x 96 DPI)

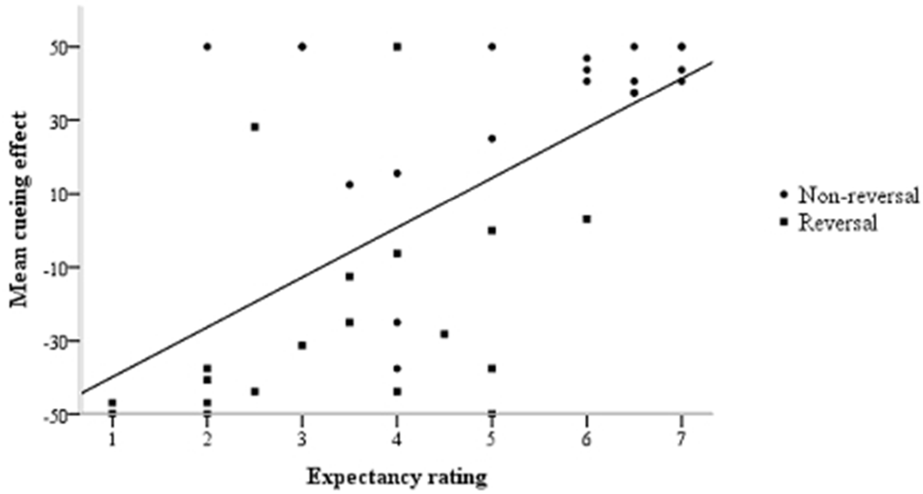

Figure 7. The positive correlation between expectancy ratings for the cued outcome and the overall cueing effect in each instruction group.
129x67mm (96 x 96 DPI)